period they decided to be false to their obligation is a matter of secondary moment.

The implication that the Allies were contemplating the use of poison gas as early as September, 1914, is so notoriously at variance with truth that it scarcely needs serious refutation. If Geheimrat Haber could only have been present when Lord Kitchener made his dignified protest in the House of Lords, or have been a personal witness of the wave of indignation and disgust which swept over the country at the Germans' breach of the rules of war, he would not have penned his statement. We were made aware of the rumours that were being spread through the German Press, but no credence was attached to them in this country. The German Empire, even to the last, had its "reptile Press" as in the days of the Iron Chancellor. The author of the Ems telegram was an adept in the art of circulating false rumours and misleading statements, and there were those who sought to better even his example during the fateful and, for the Germans, disastrous years of the war. Even now Germany does not know half the truth.

I agree with Prof. Haber that in war-time men are apt to think otherwise than they do in peace, and this is, doubtless, particularly true of his countrymen. Their ethical standard, apparently, varies with the two conditions—which seems a sufficient reason why they should abstain from war. Those who use poison gas are not "bonnie fechters."

The menace of the continued use of poison gas in warfare is a disgrace to civilised humanity. That menace really rests with Germany. If she would undertake for the future to be true to her obligation under the Hague Convention other nations would willingly follow her example.

They were reluctantly compelled to follow it in consequence of her action at the Battle of Ypres. They would far more promptly follow her lead if she announced her intention to discontinue the practice and gave the world a sufficient assurance of good faith.

Prof. Haber could render no greater service to civilisation and humanity than to use his great influence and pre-eminent position as a man of science in inducing his fellow-countrymen to remove what is a stigma upon their *Kultur*.

T. E. THORPE.

Some Problems in Evolution.

I AM quite as averse from "wasting time in endless and futile controversy" as Prof. Goodrich, but I cannot help thinking that so long as he and Sir Archdall Reid refuse to admit what seem to most biologists obviously true statements their arguments must be answered. Prof. Goodrich states in NATURE of December 22 last that there is no contradiction between his proposition that characters, whether new or ancient, may be inherited provided they are possessed by both parents, and my reply that a character may be inherited when it is apparent only in one parent or in neither. If we omit what is common to both of these propositions it follows that in Prof. Goodrich's opinion there is no difference between "both parents" on one hand and "one or neither parent" on the other.

Prof. Goodrich complains that I do not distinguish between the variation and the resulting character. In his presidential address he maintained that the word "variation" should mean the extent or degree of difference between individuals, not a new character or assemblage of characters, such as a colour or spot on a butterfly's wing, but a difference which can be measured or estimated. "We shall then find," he writes, "that a variation is either due to some change in the complex of germinal factors or to some change in the complex of effective environmental stimuli." Here Prof. Goodrich and I are in complete agreement. Where, then, is any difference of opinion or room for controversy? The difficulty reappears in the next paragraph of Prof. Goodrich's letter, in which he states that he agrees with Sir Archdall Reid that there are two kinds of variation but only one kind of character.

Although I have honestly tried to do so, I fail to see any meaning in this statement. After all, words and terms are seldom completely satisfactory expressions of what we mean; they obtain their meaning by actual or implied reference to facts of observation. I have lately been in the habit of using the term "character" more frequently than the term "variation," because the latter is often ambiguous, but I know of no such difference of meaning between the two terms as is assumed by Prof. Goodrich and Sir Archdall Reid. How can we define characters satisfactorily except by comparison—that is, as differences between one individual, or one type, and another? For example, the rose comb is a character of certain fowls as compared with the single comb. What do we gain by asserting that the difference between the rose comb and the single is a variation, but the peculiarity of the rose comb is a character? The only questions of importance are the origin and the heredity of the rose comb. We understand one another when we say that the rose comb is inherited. It is quite superfluous to insist, as Sir Archdall Reid does, that characters are not transmitted, but only predispositions. No one supposes at the present day that the fowl's egg or spermatozoon has a rose comb, but we know that there may be something, whether we call it determinant, factor, or gene, in egg or sperm which causes the rose comb to develop in the resulting organism. And yet Sir Archdall Reid argues as though it were a remarkable discovery that characters are not present as such in the fertilised ovum from which an organism develops—an idea that has been obsolete since the Middle Ages. Therefore, he asserts, there is only one kind of character, but there are two kinds of variation. The hoof of a new-born foal has developed without any external mechanical stimulus; when I practise rowing for some time I develop epidermic corns on the palms of my hands. According to Sir Archdall Reid, these are characters of the same kind, equally innate, acquired, and inherited. Yet he has himself insisted on the distinction between characters developed under the "stimulus of nutrition" and those developed under the "stimulus of use," the same distinction, with his own peculiar misuse of the word *stimulus*, which is generally recognised by biologists.

Avoiding altogether the use of the terms "variation" and "character," we may congratulate ourselves that there is agreement on the proposition that a difference of form or structure may be due either to a difference of germinal factors or to a difference in effective environmental stimuli. And then we can get on with the investigation of the problem of the relation to evolution of these structural differences.

But, as I have endeavoured to show elsewhere, among those characters which are more or less completely hereditary there are two kinds, in a vast number of cases definitely distinguishable: the adaptive characters on one hand and the non-adaptive on the other. The adaptive characters exhibit a definite relation to habits and external conditions, and, as a rule, they exhibit recapitulation in development. The

© 1922 Nature Publishing Group

non-adaptive characters show no relation to differences of habit or environment, and, as a rule, develop directly without recapitulation. I have instanced the adaptive characters of Pleuronectidæ (flat-fishes) on one hand and their specific and generic characters on the other. The adaptive characters of flat-fishes exhibit one of the most remarkable cases of metamorphosis and recapitulation in the whole field of zoology, while the various peculiarities of the scales, as examples among specific and generic characters, show neither recapitulation nor any relation to habits and conditions of life. Thus, instead of agreeing that there is only one kind of character, I find it necessary to distinguish three kinds, one due to the effect of an external stimulus on the individual, and not apparently inherited, and two kinds which are hereditary.

J. T. CUNNINGHAM.

Chiswick, December 31.

Optical Observation of the Thermal Agitation of the Atoms in Crystals.

ACCORDING to the theory of specific heats developed by Debye, Nernst, and others, the thermal energy of a solid is made up of the energy of elastic vibrations in its material, the frequencies of such vibrations ranging from very small values up to a maximum limit determined by the ultimate molecular or atomic structure. On this view it is clear that at ordinary temperatures the density of a solid, and therefore also its refractive index if it be of transparent material, would vary arbitrarily from point to point about its mean value. In other words, a transparent crystal cannot be regarded as optically homogeneous even with reference to the comparatively long waves which constitute ordinary light. It follows that a certain proportion of the energy of a beam of light traversing the medium would be deviated laterally and appear as scattered light, the intensity of such scattering being a measure of the thermal agitation within the crystal. That some such effect must occur has already been pointed out by Sir Joseph Larmor (*Phil. Mag.*, vol. 37, p. 163, 1919), but no theoretical discussion of its magnitude appears so far to have been put forward. It has occurred to the present writer that the effect to be expected may be found in the following way:—If the principles of statistical mechanics and the equipartition of energy were applicable in the case of solids, precisely the same considerations which determine the molecular scattering of light in fluid media would enter here as well, and the scattering coefficient would be given by the Einstein-Smoluchowski formula

$$\frac{\pi^2}{18} \cdot \frac{RT\beta}{N\lambda^4} \cdot (\mu^2 - 1)^2(\mu^2 + 2)^2,$$

where β is the compressibility of the solid, μ is its refractive index, λ is the wave-length of the light, and R, T, N are the constants of the kinetic theory. It is known, however, that the heat-content of solids at the ordinary temperature is much less than that indicated by the equipartition principle, the deficiency being most marked for substances, such as diamond, having a high "characteristic temperature." The scattering coefficient given by the preceding formula must therefore be diminished in the ratio which the actual heat-content at the temperature of observation bears to the heat-content indicated by the equipartition principle. This correction-factor may be found from the experimental data for the specific heats at low temperatures given by Nernst, Lindemann, and others.

Calculations made in the way indicated above show that transparent quartz should scatter light $9\frac{1}{2}$ times as strongly as dust-free air at normal temperature and pressure. A scattering of approximately this magnitude in clear quartz was detected photographically by R. J. Strutt (now Lord Rayleigh) (Proc. Roy. Soc., vol. 95, p. 495, 1919), but was ascribed by him to inclusions which he assumed were present in the crystal. It is clear from what has been said above that the effect observed by him was actually due to the thermal agitation of the atoms in the crystal. The present writer has succeeded in demonstrating the scattering of light in clear quartz by direct visual observation. For this purpose a block of the crystal with smooth polished faces is immersed in a tank of clean distilled water to minimise surface-reflections and a converging lens is used to bring a beam of sunlight to a focus within the crystal. The blue track of the beam within the crystal may then be readily observed, and its intensity can be judged by comparison with the scattering of the beam in saturated ether vapour. The writer has had the pleasure of exhibiting the phenomenon to Sir W. J. Pope and other distinguished callers at his laboratory.

Transparent rock-salt which has a low characteristic temperature and shows a marked "Debye-effect" in experiments on X-ray reflection exhibits a very strong scattering of ordinary light. The increase of the scattering with rise of temperature may readily be observed with it.

C. V. RAMAN.

210 Bowbazaar Street, Calcutta, November 19.

A Fossil Buttercup.

WHEN we examine a catalogue of fossil plants, such as that for North America recently published by Knowlton, we are struck by the enormous number of recorded species, and readily receive the impression that the flora of former ages is quite well known. It is only when we make a more critical investigation that we perceive the great gap in our present knowledge. We do, perhaps, know a fair proportion of the trees and deciduous-leafed shrubs of a number of geological periods, but when we look for the herbaceous flora the limitations of our knowledge at once appear. Thus the Ranunculaceæ, an extensive family in the present North American flora, do not furnish a single definitely recorded fossil in the same area. Dawson in 1875 vaguely referred to a Thalictrum, without specific name, supposedly from the Eocene, but it is not to be taken seriously. Schenk thought the fossil genus Dewalquea presented a certain analogy with Helleborus, but it is now referred to quite another family. It is, of course, impossible to suppose that the Ranunculaceæ were absent from North America during Tertiary times; they simply must have escaped preservation or observation. To those who would see in the geological record a proof that herbaceous plants did not exist in the past, or were extremely rare, we can only reply that the record as it stands proves too much. To accept it at its face-value postulates the impossible. The general proposition that the herbaceous flora is, *on the whole*, more recent than the woody may be valid, and has much to recommend it.

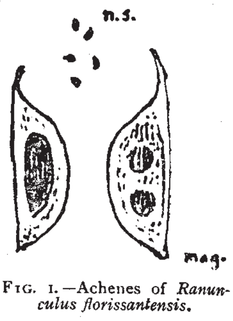

FIG. 1.—Achenes of *Ranunculus florissantensis.*

With regard to the Tertiary Ranunculaceæ of North America, we can fortunately rescue them from utter oblivion. Several years ago I found some slabs of Miocene shale at Florissant, Colorado, plentifully besprinkled with small dark fossil seeds. The exact locality is the railroad cut just east of the town.

© 1922 Nature Publishing Group